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Veröffentlichungsversion / Published Version

Zeitschriftenartikel / journal article

Empfohlene Zitierung / Suggested Citation:

Ringen, S. (2006). The Truth about Class Inequality. *Sociologický časopis / Czech Sociological Review*, 42(3), 475-492. <https://nbn-resolving.org/urn:nbn:de:0168-ssoar-56517>

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The Truth about Class Inequality*

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Abstract: A strongly recommended conclusion in sociology about trends in class inequality has been summarised by Goldthorpe as a high degree of 'temporal constancy and cross-national communality'. This conclusion, here called 'the stability thesis', was first challenged by Ringen in 1987 and again, on more methodological grounds, by Ringen and Hellevik in two papers published in 1997. These challenges resulted in a process of debate and reassessment. It is now possible to sum up and conclude. The stability thesis rests on empirical results from odds-ratio readings of mobility table data. The authority of this methodology is re-examined in terms of normative significance and statistical validity. Mobility table data which have generated stability thesis findings are re-analysed with the standard gini-index methodology in the study of inequality, then yielding different findings which contradict the stability thesis. The main conclusion is that the stability thesis can now be considered overturned.

Keywords: social inequality, social justice, social reform, class analysis, social stratification.

Sociologický časopis/Czech Sociological Review, 2006, Vol. 42, No. 3: 475–491

In a paper published in *Citizens, Families and Reform* [Ringen 1997], I challenged what had become a strongly recommended conclusion in sociology about trends in class inequality (in a body of literature that for convenience I will refer to as 'the class inequality literature'). The paper was a product of three years of collaborative work with Ottar Hellevik. A separate paper by Hellevik with a similar criticism was published simultaneously in *Acta Sociologica* [Hellevik 1997]. These criticisms resulted in a process of debate and reassessment [Marshall and Swift 1999; Hellevik 2000; Ringen 2000; Marshall and Swift 2000; Swift 2000; Kivinen et al. 2001, 2002; Hellevik 2002b; Ringen 2005b]. It is now possible to sum up that debate and reassessment and offer some conclusions.

* This is a revised version of a paper presented to the Department of Sociology seminar at Oxford University on 31 October 2005. As ever in this project I am grateful to Ottar Hellevik for close and fruitful co-operation.

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The stability thesis

In the first edition of *Social Mobility and Class Structure in Modern Britain* [Goldthorpe et al. 1980], the authors followed trends in class inequality and social mobility over a period of roughly fifty years leading up to 1980. The final conclusion was that in spite of a massive thrust of upward social mobility 'no significant reduction in class inequalities was in fact achieved'. That conclusion was repeated in the second edition of the book [Goldthorpe et al. 1987: 328]. This I call 'the stability thesis'.

The stability thesis was subsequently confirmed and re-confirmed in further analysis on British trends: '... the terms of the competition between the classes has shown little sign of change' [Heath and Clifford 1990: 15]; '... we have found that substantial absolute rates of upward and downward mobility coexist alongside relative class mobility chances which have remained largely unchanged ... in most of [the twentieth] century up to the present day' [Marshall et al. 1997: 59]. In a new development, the thesis was found to apply not only over time within countries such as Britain but also in comparisons between countries, including between the European countries with recent historical experience of democracy and those with experience of communist authoritarianism: '... in most countries there has been little change in socioeconomic inequality of educational opportunity [Shavit and Blossfeld 1993: 19]. Across advanced societies '... there are more opportunities for mobility now than in previous decades. However, the distribution of these enhanced opportunities across classes is quite another matter' [Marshall et al. 1997: 3]. In *On Sociology*, Goldthorpe argued that years of '... technical advances in social mobility research have ... led to empirical findings on temporal constancy and cross-national communality' in the relevant kind of class inequality, and that this 'might be regarded as the main achievement to date of class analysis as a research programme' [Goldthorpe 2000: 163, 257].

Much is obviously *not* at issue: the existence of class inequality in Britain and elsewhere, the fact of upward social mobility over time in Britain and elsewhere, the fact of social difference in many dimensions between European and other countries. Rather, the stability thesis says that, in spite of everything that has changed over time and everything that is different between countries, one thing at the heart of social life has remained remarkably stable over time and constant in space, and that is class inequality.

The challenge

Class analysis is, among other things, an analysis of social inequality through the prism of class. My background was a different one: the analysis of social inequality through the prism of income distribution and poverty [Ringen 1987, 1988, 1997; Ringen and Uusitalo 1992]. My research has been, as is the class analysis in question, on trends over time and differences and similarities in space and has coincided with the class analysis in the periods covered and the kind of countries com-

pared. It is also similar in that it relies on the analysis of large data sets, often data sets carefully rearranged for comparative robustness. However, my conclusions were totally at odds with those of the class analysts. I had consistently recorded changes over time and differences in space. That record is confirmed in other work on income distribution [e.g. Atkinson 1995; Atkinson et al. 1995]. In poverty research, a separate stability thesis was suggested, starting with Townsend's *Poverty in the United Kingdom* [1979] and in further work under that inspiration (incidentally, inspired by a concept of relative poverty, which has many similarities to the concept of relative class inequality that has been used in parts of the class inequality literature [e.g. Erikson and Goldthorpe 1992]). However, that proposition has been effectively refuted, and the evidence now firmly indicates that poverty rates typically show patters of change over time within countries and of difference between countries, including rich countries [Ringen 1988, 1997; Atkinson 1998; UNDP annual; UNICEF 2005] (see also www.lisproject.org). The body of literature I refer to here I will for convenience call 'the income inequality literature'.

The class analysis in question is highly technical in methodology. For its conclusions it rests crucially on the reading of mobility table data through odds-ratios. A major advantage of that methodology is argued to be '... that these ratios constitute the elements of log-linear models' [Erikson and Goldthorpe 1992: 56]. Hellevik's background for our joint project was as a methodologist and as the author of textbooks in statistical methodology [Hellevik 1984, 2002a]. He was sceptical of some uses of log-linear regression analysis in sociology, which he saw as a complicated and impenetrable way of doing what could usually be done using simpler and more transparent techniques. Although for a reason other than mine, he was therefore also distrustful of the research underpinning the stability thesis.

Hellevik and I resolved to join forces to re-investigate the class inequality research and the stability thesis. This research had long been under criticism from others [e.g. Pahl 1989; Sorensen 1991; Holton and Turner 1994; Crompton 1996; Saunders 1990, 1995, 1996]. However, much of that criticism, certainly from fellow sociologists in Britain, had failed to penetrate the methodological complexities of the research being targeted and had therefore had limited impact. We decided to go back to the basics and explore the methodological foundations.

Empirical research on class inequality and mobility consists in the reading of mobility tables. If we are interested in trends or differences in inequality, which is the issue here, we need to observe consecutive mobility tables to see if some display less or more inequality than others. Figure 1 shows two simple mobility tables of class background and educational attainment for two imaginary populations of 1000 people. The question here is whether different displays of data such as these show the same or a different degree of inequality.

A mobility table contains two sets of distributional data. The first set consists of two marginal distributions, say, the proportion of the population with a working class background, a middle class background, and so on, and the proportions that finish education at the basic level, that go on to secondary education, and so on. The

Figure 1. Two class-education mobility tables, imaginary populations

	Table A O-R association = .58 Gini distribution = .26			Table B O-R association = .58 Gini distribution = .17		
	Low educ.	High educ.	N	Low educ.	High educ.	N
High class	100	200	300	48	252	300
Mid. class	250	150	400	156	244	400
Low class	250	50	300	197	103	300
N	600	400	1000	401	599	1000

O-R association = $\frac{1}{4}\ln(\text{odds-ratio high class/low class})$.

Gini distribution estimated from Lorenz curve of share in higher education to each class.

Source: Hellevik [1997], Ringen [1997].

second set consists of the conditional distributions in the space defined by the two marginals, say, those originating in the working class and finishing education at the basic level, those originating in the middle class and going on to university education, and so on.

Marginal and conditional distributions are related. If something changes in the marginal distributions, something has to change also in the conditional ones. If, for example, the number of places in higher education is increased and those places are taken up, the new students have to come from somewhere, so that a higher proportion of people from at least one social class, possibly several or all, will end up in higher education.

Everyone agrees that mobility tables contain data on inequality, but the story of inequality cannot be read directly out of the table, the data need to be interpreted. For example, if again the number of places in higher education is increased and new students are recruited from all classes by background (as in Table B compared to Table A in Figure 1), is the result more or less inequality?

The interpretation of mobility tables that is behind the stability thesis is based on the following logic. The marginal distributions are seen to display social structure, class structure and the structure of education in our case. Inequality, or at least some kind of inequality, lies not in the social structure but in how people are distributed within that structure. Therefore, only the conditional distributions contain data on the relevant kind of inequality. Furthermore, some of the information contained in the conditional distributions is really structural data in disguise, for example, the effect in the conditional distributions that follows directly from changes in the marginal distributions. Therefore, the correct reading on trends in inequality is to identify changes in the conditional distributions after 'subtracting' or controlling for those changes that follow directly from changes in the marginals: '... the pattern of association net of the effects of the marginal distributions' [Erikson and Goldthorpe 1992: 56]. For example, if yet again the number of places in higher edu-

Figure 2. Class association and class distribution of higher educational attainment in Britain by birth cohort

	Birth cohorts			
	1930–39	1940–49	1950–59	1960/later
O-R association	.46	.43	.50	.44
Gini distribution	.24	.20	.15	.11

Higher education = O-level equivalent and higher. The proportion in the cohorts (all classes) attaining that level of education increased from 28% in the 1930–39 cohort to 68% in the 1960/later cohort.

Source: Heath and Clifford [1990], Hellevik [1997], Ringen [1997].

cation were to increase, there would by this logic be no effect on the relevant kind of inequality unless one could identify changes in the conditional distributions beyond those that come directly as a result of the added number of places. The odds-ratio technique is said to be ‘margin-insensitive’. (The methodology is explained in Chapter 2 of Erikson and Goldthorpe [1992]) In the example in Figure 1, both tables by this measure show identical degrees of inequality.

This may look impressive, but it is in fact a bizarre way of reading mobility table data for the purpose of interpreting inequality. If we are interested in changes in inequality, why remove from our vision certain changes in inequality because they are changes that have a certain cause, in this case changes in inequality which follow directly from changes in the social structure? Why not do the obvious thing and read the whole picture? The odds-ratio reading appears partial. Intuition suggests that it would be safer to interpret the message contained in the table through a complete analysis that reads all the data.

That is what we proceeded to do. Instead of odds-ratios we adapted the standard technique in the income inequality literature of gini-indices estimated from Lorenz curves to the analysis of mobility table data. (The technique is explained in Hellevik [1997] and Ringen [1997].) That reading interprets mobility table evidence by taking note of both marginal and conditional distributions. As can be seen in Figure 1, this reading shows there to be less inequality in Table B than in Table A.

From these examples we moved on to real data. We took the case of class background and educational attainment, one of the associations that had been found to remain roughly stable over decades in Britain [Heath and Clifford 1990]. We re-analysed the British mobility data in which our colleagues had read stability of class inequality. We kept everything the same except the statistical technique. We read the same mobility tables with the same data for the same country for the same period.

As can be seen in Figure 2, the two ways of reading the same data give totally different results. In the period covered, the overall level of educational attainment in the population increased sharply. While the odds-ratio reading shows stable in-

equality (or at least no clear trend up or down), we saw in our reading of the same data a straight, sharp and remarkable reduction in class inequality of educational attainment.

What then is the true story of class inequality, stability, and similarity or change and difference?

Why the question matters

The terms of the debate were set in my book *The Possibility of Politics*. Here I observed, by way of conclusion, that my findings '... count as rather encouraging ones for the strategy of seeking to attack social inequalities via legislative and administrative measures of a piecemeal kind' [Ringen 1987: 207]. That was a riposte to the first edition of *Social Mobility and Class Structure in Modern Britain*, in which the authors had observed that their findings '... count as rather grave ones for ... the strategy of seeking to attack social inequalities via legislative and administrative measures of a piecemeal kind' [Goldthorpe et al. 1980: 252]. This debate is above all about substantive issues in social policy. Crudely, if the stability thesis is true, the welfare state is in vain.

The class inequality literature has produced a string of dismal conclusions to that effect, following the lead of *Social Mobility and Class Structure in Modern Britain* in 1980. For example, with support in the stability of odds-ratios as reproduced in Figure 2, the authors concluded: 'Neither the meritocratic reforms of the 1944 Act nor comprehensive reorganisation can, in this respect at least, be said to have succeeded' [Heath and Clifford 1990: 15]. And further, '... the advantaged social classes have been able to outmanoeuvre the social reformers.... Legislation may change the rules of the game, but the players who are most motivated to succeed may be able to adapt their strategies so that they succeed in the new game just as they did in the old' [Heath et al. 1992: 220, 241]. The consequence '... of post-war economic growth was not that it facilitated egalitarian reform but rather that it obscured its failure. The reformers underestimated the resistance that the class structure can offer to attempts to change it: or, to speak less figuratively, the flexibility and effectiveness with which the more powerful and advantaged groupings in society can use the resources at their disposal to preserve their privileged positions' [Goldthorpe et al. 1987: 328]; '... the post-war project of creating in Britain a more open society, through economic expansion, educational reform, and egalitarian social policies, has signally failed to secure its objective' [Marshall et al. 1988: 138]; '... the impact of educational reforms on changes in educational stratification seems to be negligible. Nowhere have they reduced inequalities of educational opportunity between socioeconomic strata' [Shavit and Blossfeld 1993: 21]; 'Have mobility opportunities in these societies become more equal in these societies? The findings ... suggest that the answer to this question is in all four cases negative' [Marshall et al. 1997: 57]. (The cases/periods being West Germany 1976–91, Poland 1972–91, Czechoslovakia 1984–91 and the USA 1973–91.)

Beyond policy implications, methodological questions arise. When two methods for the analysis of the same data give different results, there is something unresolved that needs to be sorted out. In this case, two bodies of literature on social inequality tell contrasting stories. Either we find a way of reconciling those differences or we must conclude that the best available social research is inconclusive on major social trends.

Finally, there are broader theoretical implications. There is one outlook on modern social history that may be referred to as 'the liberal theory', which by and large assumes that macro-sociological progress is genuine, for example, that economic modernisation tends to follow through to improved life chances for more people. One expression of this theory is a belief that social mobility is equalising in that it causes class-linked inequalities of opportunity to be steadily reduced. This is the main competing view to the one under discussion here and to the broader theory that is under attack from the stability thesis school. 'If the foregoing results are sound, they cast serious doubts on the arguments advanced by liberal theorists of industrialism' [Marshall et al. 1997: 54]. In *On Sociology* (especially in Chapter 8), Goldthorpe dismisses the liberal theory (as represented by, for example, Blau and Duncan [1967], Treiman [1970] and Kerr [1983]) on the argument, again underpinned by stability thesis observations, that its predicted 'withering-away of class ... has yet to be observed' [Goldthorpe 2000: 163]. What then remains is a dark undertone not only about the futility of the welfare state but more basically about the very idea of progress.

These claims on odds-ratio evidence are in hindsight astonishingly strong. The stability thesis is treated as an established fact, so much so that not only is it the 'main achievement' of class analysis, but it is an achievement of such dignity that 'the focus of theoretical effort in the field should now be ... to explain' that constancy and communality [Goldthorpe 2000: 257]. The implications for social policy are 'grave', egalitarian reform has generally been a 'failure', whole post-war projects have 'signally failed', and across a range of countries educational reforms have 'nowhere' reduced inequalities. No less than the liberal theory in all its glory stands naked and accused.

All this rests on a single and specific way of reading mobility tables and on that methodology alone: through odds-ratios. Similar strong claims have been made for that methodology. Log-linear models '... have emerged as the sociologist's most flexible yet powerful means for the analysis of mobility tables [and represent when] applied ... to investigate the more detailed features of relative rates [a] technical innovation' [Erikson and Goldthorpe 1992: 56–7]. This is a methodology that makes it '... possible to specify the intrinsic association between variables after purging out nuisance variability in marginal distributions' [Grusky and Tienda 1993: vii]. 'Under the logistic response model, differences in background effects ... cannot result from changing marginal distributions of either independent or dependent variables because such changes do not affect the [measure]' [Mare 1981: 75]; '... logit models [reveal] the "pure" association between origin characteristics and educational attainment [Breen and Jonsson 2005]. What is said to give this methodology its unique-

ness of power is that it produces 'margin-insensitive' measures of origin-destination associations.

Where the confidence in this methodology alone and the importance of its findings comes from is something of a mystery. It is a very difficult and convoluted way of reading data and there are very good reasons to mistrust the methodology both on its measurement validity and its normative significance.

One thing is presently *not* at issue: the usefulness or power of log-linear analysis in general, or of any other statistical technique for that matter. This is less a discussion about statistics than about the *usage* of statistics. (But for follow-ups on logistic versus linear regression techniques in general, see Hellevik [2003, 2005].)

Normative significance

The value reference shared by everyone in this debate is egalitarianism. The Hellevik–Ringén reading of mobility table data is easily interpretable and not controversial. Provided the destination side of the table represents attractive positions, it is a measure of class inequality in the attainment of those positions. From an egalitarian perspective, this is information of obvious normative significance. Class inequality thus measured persists in the data referred to here, but not on a stable level. Figure 2, for example, shows it to have been reduced in line with the extension of educational opportunities.

The odds-ratio reading is, however, anything but easy to interpret. Just what is it that is being measured by odds-ratios in class mobility tables?

The answer has been a bit unclear and shifting. It was first thought that a particular reading of *class* inequality was sufficient for general pronouncements on *social* inequality. This was the interpretation in the first edition of *Social Mobility and Class Structure in Modern Britain*, hence the grave implications for 'social inequalities'. This position was, however, abandoned, even in the second edition of that book. Class inequality has now become, more modestly, precisely a matter of *class* inequality. In the next stage, following the results of Hellevik's and my re-analysis, and in the exchange that took place over those results with Marshall and Swift, that position also was abandoned in favour of a yet more modest view, according to which there are several kinds of class inequality and both ways of reading mobility tables are correct, albeit in respect to different kinds of inequality.

That is a step towards clarification, but only a first step. In a subsequent paper, Adam Swift goes back to methodological first base yet again and asks 'precisely what it is that class analysis using odds ratios does and does not tell us' [Swift 2000: 664]. Here he underlines the 'limitations' in this kind of class analysis and how it is a 'narrower, or more carefully specified, research programme than some had realised' [ibid.]. The consequence of this clarification is that odds-ratio readings are interpreted so as to not tell us much or anything at all of normative significance about inequality: '... everybody can be getting better off, there can be more chances of upward mobility, the gaps between the positions that members of the ... groups

tend to end up in can be getting smaller, the distribution of opportunities to achieve absolute levels of goods can be getting more equal. More equal also can be both the distribution of opportunities to achieve [a specified standard of living] and the distribution of opportunities to buy goods. All this can happen without any increase in social fluidity between class positions' [ibid.: 667].

If that adds some further clarification to what is being measured, the next question is what it really *means*. 'I have attempted to justify the normative significance of the findings of mobility analyses ... involving odds ratios. But I've not sought to conceal the complexities involved in such a justification. ... To those who care about equality, but who also care about the distribution of things like opportunities to buy goods, social fluidity is an important part of the story. But it is only a part. A complete analysis of the distribution of *those* opportunities would involve reference to the size of the gaps between class positions as well as the extent of movement between them' [ibid.: 675]. In other words, the 'complete analysis' would be the kind of analysis Hellevik and I conducted, which consists of reading all the distributional data in the mobility table. For the rest, the 'important part of the story' is that odds-ratio readings tell us *something* about *some* differences in *some* kinds of opportunity. Not about social inequality as such, not about class inequality as such, not even about class inequality of opportunity as such.

It now turns out, according to Swift, that this research programme was never about inequality in a normative meaning after all, or at least not primarily so, but that it was always 'explanatory rather than normative'. What then gives odds-ratio readings significance is that they 'contribute to the explanation of these class-related phenomena' [Swift 2000: 676].

According to this interpretation, the two series of statistics in Figure 2 would seem to be not alternative readings of class inequality, not even different kinds of inequality, but one reading on inequality and one on an element in the explanation behind it. Together these data would then show a reduction in class inequality in spite of no weakening of the force of one contributing cause of inequality.

But that is not Swift's interpretation. He finds that the odds-ratio reading uncovers a contributing explanation, but at the same time he interprets it as a measure of inequality, albeit only one kind of inequality. This one thing is both an explanation of inequality and itself a kind of inequality.

The logic would appear to be the following. Young people go through their early lives in search of education (or other attractive positions). We know that of those who started out in a lower social class a smaller proportion attained a higher level of education than those who started out in higher social classes. That difference is explained, in part, by what is measured by odds-ratios. What might justify calling that 'class inequality' would be a credible assumption that those who move along the road from origins to destinations with the label of 'lower class' are treated or affected differently in that process from others in ways that make it more difficult for them to attain the desired position or destination. Inequality is a normative concept. It refers to differences that are objectionable, in our case from an egal-

itarian point of view. If young people from lower social classes are blocked from fulfilling their aspirations, that would indeed be a case of class inequality.

However, to justify that conclusion and interpretation it is not enough to establish that what odds-ratios tell us has the force of a contributing explanation of class-related phenomena. It needs to be established that it is a special kind of explanation. Swift's conclusion is persuasive as far as it goes, but he has only established that we are on to an explanation of class inequality, not that it is a class inequality explanation of class inequality.

In a contingency table, a measure such as the odds-ratio uncovers whether there is a statistical association between the independent and the dependent variables and suggests the strength of that association. That there is a statistical association between class of origin and educational attainment in the displays in Figure 1 and in the real British data in Figure 2 is clear and obvious. But it is *not* clear and obvious that this association is a case of class inequality.

From analyses of mobility tables we are able to identify associations descriptively but nothing more. We may well think that an observed association hides a causal link from, in our case, class to education. That inference is supported by the fact that class comes first and education later, but it is still just an inference. All we *know* is that there is an association. However carefully we analyse mobility table data, from that analysis we *know* nothing about the mechanism(s) through which the inferred causality might work. In the class inequality literature it is said to be class inequality, but that is just a postulate.

If we have a firm theory of causality, we can take contingency table associations as evidence of the causal force. For example, if the independent variable is vaccination or not and the dependent variable the incidence of the disease the vaccination is against, we can take the association between vaccination and incidence as good evidence of the effectiveness of the vaccination. We can do that, in spite of having no observations about how the vaccine works in a patient's immune system, because we are on safe ground in assuming that that is where it does its job.

In the analysis of class mobility tables, we have no such firm theory to fall back on. We simply do not know through what mechanism(s) class background is supposed to cause differences in educational attainment, or at least there is no theoretical agreement on this.

A mobility table is a simple organisation of data: marginal and conditional distributions clearly laid out. Those data as they appear on a sheet of paper, say, in Figure 1, or as they usually appear with more finely grained decompositions of the marginal distributions, can be analysed with great statistical precision. That is certainly being done in the class analysis literature, with ever increasing technical sophistication.

However, the simplicity of the mobility table is deceptive. An organisation of data that looks simple on paper is in effect a snap-shot compilation of information about life-course movements over a very long period. The mobility table looks two-dimensional, but in fact there is a third dimension in it, the dimension of time. If

the destination is educational attainment, there may typically be fifteen to twenty or twenty-five years between the positions of origin and destination. If it is occupational attainment, the table may cover a span of forty to fifty or sixty years. Over a life-course a myriad of things happen to individuals which may contribute in various ways to their success in life, for example their educational attainment.

In the class inequality literature, the assumption is that everyone is equally motivated to seek higher education but that something descends upon them in the process and shuts or opens doors to people, all depending on their class backgrounds. Halsey has likened it to 'loaded dice' and suggested that what we are reading is those loadings [Halsey 1977].

However, all we have to go by in support of that assumption is an observed association. In this case that observation does not do much for us since it does not sit on any firm theory. The same observation is in conformity with utterly different assumptions about the mechanisms that may be at work. The important alternative theory to the one postulated in the class inequality literature is that the statistical association between class and education is a result of self-selection rather than of dice or other treatments or influences manipulated by someone else. If young people originating in different classes are differently motivated for higher education, or if there are differences in ability that are correlated with class, a statistical association between class and education might well reflect *equality* between the classes in the attainment of what their members themselves aim for, or fairness of treatment according to ability.

Socially differential motivation (or ability) might still be seen as a form of class inequality if we think that a lower level of aspiration (or skill) in young people from lower social classes is itself a legacy of class, but class legacy is not the only or obvious interpretation. We might for example more modestly take it as an inescapable evolutionary fact. Or it could be the result of informed and rational choice [*cf.* e.g. Boudon 1974; Gambetta 1987].

The point here is that from the observed association we cannot tell which theory to trust. You or I might want to believe one rather than another, but the mobility table offers no help in substantiating such beliefs. If we have no firm theory of causality *a priori*, no reading of the mobility table can adjudicate afterwards between alternative possible theories.

This is by and large a re-statement of prior criticism by, in particular, Peter Saunders [Saunders 1990, 1995, 1996]. That criticism, however, has pretty much been ignored in the class inequality literature (although not by Swift [2000]). Goldthorpe, for example, in *On Sociology*, where the stability thesis is 'the main achievement to date of class analysis', deals with it by not mentioning it.

To sum up so far:

1. Yes, it is established that there is class inequality of educational attainment (which of course comes as no surprise to anyone).
2. Yes, it is established that there is an association between class and education as measured in mobility tables (which is also pretty obvious).

3. No, it is not established that a class-destination association is a case of class inequality in a normative meaning. That has been postulated but not established.
4. Hence, the only established evidence on class inequality from mobility table analysis is that generated by Hellevik–Ringen type readings.

Validity

A final mystery now remains to be solved. If the odds-ratio measures a cause of inequality and the Hellevik–Ringen reading the resulting distributions, how can the force of the cause have been stable when the result has changed? The question has perhaps become a rather academic one of little substantive importance since we now know that the class-destination association does not contain established independent evidence on class inequality. But it still merits consideration, not least because this is where the whole discussion started.

We are forced back yet again to the question of what odds-ratios measure. If we believe that they measure the mechanism in the process from origins to destinations that produces class inequality in the distribution of attainment – in other words, Halsey's loading of the dice – the answer is that the cause cannot have maintained stable force when the resulting distribution has changed. That is simply mathematically impossible. If the loading of the dice is the same, the result will be the same. If the result changes, the loading of the dice will have changed. No difference in wording can get around that obviousness. If instead of 'loaded dice' we believe, for example, in the 'class bias of selection', 'relative inequality', 'relative opportunity', or 'fluidity', the necessary conclusion is the same.

However, the combination of a stable causal force and a changing distributional outcome is not totally impossible. If the odds-ratio measures not the mechanism but the force of one among several causes that together produce the distributional result, the force of *that* cause could still be stable. That is perhaps Swift's interpretation, something that underlines again the 'narrowness' of the research in question. If anything, then, what has been established is the stability of one contributing cause of class inequality, which on the one hand is not a class inequality explanation, and on the other hand matters next to nothing for class inequality in the final distribution.

But in truth this solution, even if not impossible, feels outlandish and incredible. For example, going back to Figure 2, it would seem entirely implausible, in the face of the changes in the class distribution of educational attainment displayed there, to suggest that class inequality of opportunity has nevertheless been unchanging, yet that is the standard interpretation of data such as these in the class inequality literature. The obvious starting hypothesis must be that when the class distribution of a good changes, the class-destination association will have been modified. We should want pretty solid evidence to overturn a hypothesis of such apparent obviousness. No evidence of such persuasiveness is, however, provided. First, the narrowness of evidence on offer does not seem to be persuasive even on

its own terms. Second, even the narrow evidence as it stands does not serve to disprove the intuitive hypothesis. The reason is that odds-ratio evidence is not proof of stability, or not in the class-destination association.

There is undisputed evidence of the stability of odds-ratios in time and space. However, just as class-destination evidence is not sufficient to establish the facts of class inequality, odds-ratio evidence is not sufficient to establish the facts of the class-destination association. The argument in the class inequality literature is that the odds-ratio method has a unique quality and power. It is supposed to tell the truth about the associations hidden in the conditional distributions in the mobility table with an authority that is superior to other ways of analysing the data. That authority comes from the idea of 'margin-insensitivity'. 'Odds-ratios are able to capture such net associations because they are margin-insensitive measures' [Erikson and Goldthorpe 1992: 56].

In fact, however, odds-ratios are *not* margin-insensitive. If margin-insensitivity is at all a meaningful statistical concept, the most that could be said is that odds-ratios are margin-insensitive *under certain conditions*. The reference in statistical theory is that '... the cross-product ratio is invariant under row and column multiplications' [Bishop, Fienberg and Holland 1975: 14]. That is something, but it falls short of meaning that margin-insensitivity is a general quality of the odds-ratio measure.

Statistical associations in contingency tables can be measured in different ways. While the odds-ratio is a log-linear measure, the difference in proportions, for example, is a linear measure of the same thing. How various measures of association behave under changing marginal distributions is a pretty open question and certainly not something that can be pinned down as general margin-insensitivity. Experiments show that both log-linear and linear measures of association sometimes respond to changes in the marginals and sometimes not, all depending on rather complicated constellations of conditions [*cf.* Hellevik 2002b; 2003]. The postulated uniqueness of the odds-ratio measure, supported by the argument of margin-insensitivity, is simply a myth that has grown out of a simple statistical observation, the meaning and significance of which has been badly exaggerated and given credence by having been repeated by authors who refer to each other. The odds-ratio is one of several available measures of association and not one that, for the present purpose, stands out from all others in authority.

If we return to Figure 2 and re-estimate the class-education associations using the difference in proportions (again between high and low class) instead of the odds-ratio, we get a different result from the odds-ratio reading. The associations across the four birth cohorts are now: .40 – .40 – .42 – .33 [Hellevik 2003]. This is not radically different from the odds-ratio results but sufficiently different to overturn the conclusion. From these data we would have been unable to conclude that the association has remained stable and we would have had to conclude that the force of the causal factor measured is weaker at the end of the period than at the beginning. There is from a methodological point of view no less reason to trust these results than the odds-ratio results.

Conclusion

Some recent class sociology in the tradition under scrutiny here suggests that confidence in the stability thesis is slipping [Breen 2004]. (See also a recent review in Breen and Jonsson [2005]). That may be a result of new observations that do not fall in line with earlier ones, or of a greater emphasis on exceptions in international comparisons, such as the case of Sweden, or variations within overall stability trends. This kind of re-assessment is welcome but is of no consequence for the present discussion. The argument here is that there never was stability of class inequality in the data as analysed, and that odds-ratio observations are not conclusive for this purpose. The same measure for the same purpose does not become more conclusive just because it begins to draw a more agreeable picture.

If we now go back to the beginning of the research programme, we see that the conclusion that 'no significant reduction in class inequalities was in fact achieved' was, from what we now know, not supported by the evidence that was referred to. That evidence contains no established independent information about class inequality. The alleged margin-insensitivity in odds ratios has turned out to be a red herring and odds-ratio evidence to have less authority than had been ascribed to it. A more correct conclusion might have been 'that no significant reduction in the power of one contributing cause of inequality was in fact achieved'. That may not be insignificant, but it is not a conclusion about class inequality – all the less so since there is now other and clearer evidence of changing inequality in the same data. In recent political parlance, excessive and unsubstantiated conclusions and inferences were drawn from a dossier that had been (inadvertently, no doubt) sexed-up.

The consequences of this reinterpretation are far-reaching. First, that original and powerful conclusion about 'class inequalities' can now be considered overturned. It is, to be clear, overturned on its own terms. It is now also in question from new findings within its own framework, but those new findings would not have been necessary to overturn the original conclusion. The more correct conclusion about 'one contributing cause of inequality' is no match. It is not the stuff of a stability thesis that anyone but methodologists, and hardly even they, would be able to get excited about. It is not a finding with grave implications for social policy or a basis for dismissing any post-war project or any other programme of reform as failures (unless one thinks that egalitarian reform is not about equality at all but only about a narrow and specific contributing cause of it, which is not a class inequality cause and which the evidence shows is easily overridden by other causes). Nor is it a launch pad for much of an assault on any competing theory of, say, openness in society or historical progress.

Second, since the odds-ratio reading does not provide established independent evidence on inequality, the Hellevik–Ringén reading, which indisputably does, stands unopposed. As a result, the apparent discrepancy between the class inequality literature and the income inequality literature is reconciled and the assault on liberal theory from one body of that literature refuted. The best available social

research speaks with one tongue on these major social trends, and both speak a language that is in conformity with the liberal theory.

To sum up, the stability thesis was never the truth about class inequality, the grave conclusions for social policy were never supported by relevant evidence, and the liberal theory was never under credible challenge. When, in *On Sociology*, Goldthorpe argues that 'the focus of theoretical effort in the field should be to explain' the observed constancy and communality, he is suggesting to class sociology that it dedicate itself to explaining what has not happened.

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